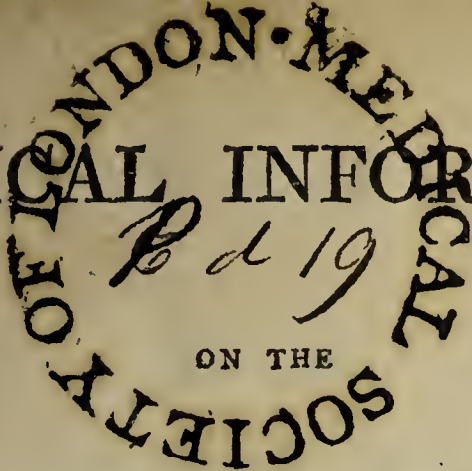


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PRACTICAL INFORMATION



MALIGNANT SCARLET FEVER

AND

SORE-THROAT.

IN WHICH

*A NEW MODE OF TREATMENT*

IS FREELY COMMUNICATED.

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# P R E F A C E.

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**I**T is common, in treating on any disorder, to give its history : to detail minutely all the symptoms which occur, whether essential and characteristic, or accidental, and common to many disorders : to treat of the diagnostics and prognostics : to investigate the remote and proximate causes ; and, then, to point out the indications of cure, and enter upon the treatment of the disease.

This regular mode of proceeding it is not my intention to follow.

The history will be given in a few words : the prominent symptoms which characterize the disorder in question, will be briefly stated ; and the simple treatment which has proved so eminently  
A 2 serviceable,

serviceable, will then be clearly described, without entering upon theory or conjecture.

*Medical theories* have been numerous : but I am afraid they have been more prejudicial than useful ; as they have, too commonly, led their converts to modes of practice, which have had no foundations, but the theories themselves : and as all those theories, being essentially different from each other, cannot be right ; of course, all the modes of practice purely theoretical, must be injurious, except those which are founded on the *truth* : and the true theory of disorders is far from being determined upon ; as is evident from the jarring opinions of the various professors of medicine, even at this present time.

It appears extremely probable to me, that one great source of error, and one great reason why real valuable knowledge is so little advanced, is, the want of strict attention and perseverance in the pursuit of truth. It has been too common a practice, and still is so, to admit of effects as causes ; and, when difficulties press, to substitute



tute words for ideas, and to adopt terms which are unintelligible, as legitimate and satisfactory.

As an example of what I have now advanced, if you cast an eye over the celebrated theory of Cullen, you will find every where *spasm*, and the *vis medicatrix naturæ*.

He makes *spasm* to be the proximate cause of fevers, and inflammations in general. Admit, if you please, that it is so; still *spasm* is not the proximate cause of those *diseases*, in which fever and inflammation occur; because *spasm* is a morbid *symptom* which arises from an adequate cause: nay, even the *cause* of *spasm* itself, does not constitute the disease, as that cause does not invariably produce *spasm*, but when it *acts on disordered parts*.

For instance, in some constitutions vegetable acids in the stomach produce restless nights, dry skin, feverish heat, and violent cramps of the feet or legs. *Spasm* is evidently the cause of the  
 pain

pain of the muscles of the extremities, and probably of the dry skin, feverish heat, and restlessness ; but this spasm itself is but an *effect*, caused by the *acid* in the stomach, which excites the *nerves* of that organ to *morbid action*, which being communicated to the *sensorium*, is productive of morbid action there ; and from thence it is propagated to the surface, and also to certain muscles of the feet or legs.

As the acid in the stomach, then, is the cause of spasm, by removing that cause, the effect will cease ; and in such cases, a sufficient dose of magnesia taken at bedtime, will prevent or cure the spasm, by neutralizing the acid, as I have experienced a thousand times.

But although the *acid* be the cause of the spasm, still it does not *constitute the disease* ; that, in fact, is that *peculiar state* of the nerves of the stomach, and of the sensorium in consequence, which renders them *subject* to *morbid actions* when acted upon by *acids*, which, otherwise, and in other states of the stomach, are *innocent*.

It may be said the *spasm* is a *morbid action*, but the *acid* is but an *exciting cause* of that morbid action. So also it may be truly said, that *fever* is a *morbid action*, and *spasm* but the exciting cause; for spasm is not fever; but it, in some cases, so deranges the actions of the system, as to be productive of that *degree of disorder*, which constitutes *fever*. In short, the nerves are liable to become disordered in *various ways*, and are subject to the action of a *great variety* of stimulants; consequently, irregular actions, spasms, and vascular disorders of *various descriptions*, must ensue.

But as spasms are excitable by *various kinds* of stimuli, acting upon nerves in *different states* of disorder, it is like saying nothing to refer all fevers and inflammations to spasm; because, to enable a practitioner to act effectually and rationally in removing spasm, he must, first of all, know what is the *nature* of the *cause*, which excites the spasm he wants to remove; and what is the *real state* of the *nerves*, which renders them thus liable to spasmodic action, in consequence of the application of such stimuli. This appears to me to be



be the only rational view of the subject : with this view I have ventured to deviate from the common modes of reasoning and of practice ; and the remedy I now propose is one result of the peculiar principles I have laid down for my medical conduct.

As spasm, then, is but an effect, it is not reasonable to confine our researches where Dr. Cullen has, by assuming spasm as the cause of fevers, and inflammations in general, without proceeding further ; for spasm, in reality, is no more than a morbid *effect*, produced by common stimuli, acting on *disordered parts*, or *morbid stimuli*, acting on sound parts, or *both* ; indeed, by thus resting contentedly with the general position that spasm is the proximate cause of fevers, &c. we have too long neglected the attempt to ascertain what are the *morbid circumstances* which *give rise* to that spasm ; and the thus admitting an *effect* as a proximate and sole *cause*, has not only involved us in error, but has kept us too long satisfied with it.

That



That substituting words for ideas, and adopting terms which are unintelligible, as legitimate and satisfactory, has been, and still is, the common practice, I need no fuller proof than Dr. Cullen's *vis medicatrix naturæ*: a set of words which give no information; of which the mind can make nothing; and which, in fact, when fairly considered, are neither more nor less than a pompous cover for ignorance.

There is another source of error, which I am sorry to say, remains, at this enlightened day, in its full force: that is, the peculiar faculty which most people have, when any inconsistency or absurdity starts into view, while they are in pursuit of a favourite hypothesis, of *turning the face aside*, or *winking* with the *eye on that side the nose*; so that they can no longer see either the difficulty which opposes, or the absurdity or inconsistency of their theories. The practice is ridiculous in the extreme; because refusing to pay any attention to an argument which militates against a favourite hypothesis, neither annihilates the objection, nor validates the hypothesis.

A proof of this occurs in the celebrated, and by many, greatly admired theory of John Brown.

According to his theory, *defect of stimulus* constitutes, in some cases, *direct debility*; and in others it induces *increased excitability*. In the former cases the quantum of stimulus must be increased, to remove the direct debility; but in the latter, the quantum of stimulus must be lessened on account of the increased excitability. But it certainly is inconsistent to assert that defect of stimulus induces direct debility, without increase of excitability, in one case; and increase of excitability, without direct debility, in the other; because, to be consistent with itself, the want of the usual stimulus ought *at all times* to produce the *same state*, curable by the *same means*, though circumstances may vary the degrees.

Dr. Darwin, in his most elaborate Zoonomia, gives nearly the same explanation of diseases as John Brown; only, instead of excitability, he uses the term—*sensorial power*. Dr. Darwin  
I says,

says, that when the sensorial power is in its natural quantity, and there is a *want of stimulus*, *direct debility* is induced : and he likewise says, that a *defect of stimulus* induces *accumulated sensorial power* ; which, by raising the stimulus even to the common standard, brings on inflammation, &c. When the disorder to be explained is really direct debility, and stimulants are wanted, then the *want of stimulus*, while the sensorial power is in due proportion, is the cause of *direct debility*. But, when violent action, or fever, or inflammation, is to be accounted for, then the *defect of stimulus*, the sensorial power being in healthy proportion, causes *accumulation of sensorial power* ; which even by the *usual stimulants*, is excited to *inflammatory action*. It certainly is an inconsistency at least, thus to attribute *contrary effects*, requiring *contrary modes of treatment*, to the *same diminution of stimulus* as a cause, acting upon *similar states of excitability*, or *sensorial power* ; and I therefore reject the systems founded upon such principles, as erroneous. Neither can I see, as Dr. Darwin seems to insinuate, that his having been led to form similar conclusions with Dr. Brown, though from

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different



different premises, is any proof of the validity of such conclusions; till it be proved that men are more prone to hit upon truisms than to draw conclusions, which do not, in the end, prove to be so valuably correct as at first they are thought to be.

It is not my intention to combat the popular systems of medicine, nor to say more than that they are imperfect and unsatisfactory, or founded upon erroneous principles; consequently to abide by them is to abandon truth; and for that reason I avoid entering upon the theory of the disease I at present have chosen to write upon; because, to adopt a language which conveys no information, or explanations founded on false principles, is to propagate error; and to support new opinions, while men are perfectly satisfied with an old one, is a sure way to be branded with the epithets of innovator, and disturber of public quiet.

For the same reasons I avoid saying any thing about the indications of cure, or the *modus operandi*

*operandi* of the medicine I recommend; for, so long as the causes of diseases are unknown, little satisfaction can be expected from the explanations which are given, of the manner in which medicines act in removing them.

That I may not, however, be thought to condemn without reason, I shall take the liberty of making a few general remarks on the popular ideas on the subject.

The *operation of medicines* in curing diseases, is, in the present theories, greatly simplified; and, in every sense of the word, the present explanations are *truly simple*.

The oxyds of antimony are powerfully emetic;—the oxyds of mercury excite salivation;—the oxyds of iron are tonic;—in fact, every metallic oxyd has its powers of action on the living system, and that by no means inconsiderable.

The *French* chemists *assert* that all these metallic oxyds produce their effects by oxydating  
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the animal fibres, or the nerves of the system, or the fluids, or all of them: therefore, the *English* chemists *assert* that they act by imparting oxygen; and those who have *no business* to *assert* at all *believe*, therefore, that all these powerful effects are produced by the oxygen of these metallic compounds being imparted to the vital fibres, &c.—Nothing can be more simple; and to those who are satisfied with simple things, nothing can be more satisfactory.

The oxygen, imparted to the system by antimony, causes *vomiting*, but no salivation. That imparted by mercury causes *salivation*, without being necessarily emetic; and the oxygen communicated by iron is neither emetic nor salivant, but simply *tonic*; unless the doses be preposterously large—then, even wine itself may act as an emetic.

Now we are taught by the French, that every metal is a simple, uncompounded body; and it is evident that neither antimony, mercury, nor iron, are possessed of active influence upon the  
body



body in their metallic states ; at least, their powers of action are incomparably less in their simple states, than when combined with oxygen ; for a man may eat a handful of iron filings, and wash them down with a gill of mercury, without inconvenience ; whereas a few grains, for instance, of mercury, when oxydated, are quite sufficient at one dose ; consequently, then, they argue, oxyds must act by means of the *oxygen* ; and it must be the *oxygen only*, which produces the powerful effects in question. The only difficulty which occurs in this matter is, to explain, intelligibly, why the *same* pure oxygenous principle should, by *mere separation* from these simple metals, sometimes prove violently *emetic*, at others powerfully *sali-vant*, and on a third occasion simply *tonic* ?

Again, quicksilver is mild, inactive, and may be drank, like water, without sensible effect.

The muriatic acid may be taken to the quantity of a *drachm* or *two*, with water, without any inconvenience.

If

If to this muriatic acid you *add* a considerable proportion of the *principle of acidity*, it will become *less acid*, and may then be taken in still *larger* quantities diluted with water, or almost without dilution !

If this simple quicksilver be put to this mild, or fur-oxygenated muriatic acid, an oxyd or oxymuriate of quicksilver will be formed ; of which, those who take *one grain*, at *once*, will most sincerely wish they had been content with *less*.

Now, if this grain of oxymuriate of quicksilver produces such violent effects by simply imparting the third part of a grain of oxymuriatic acid, which it had combined with, to the vital fibres of the stomach ; how happens it that five hundred times as much of the *same* kind of muriatic acid as that which was combined with the mercury, has no such violent effects, nor scarcely any considerable effects whatever ?

To avoid as decently as possible, difficulties like this when they impertinently intrude, as has  
been

been the case on other occasions beside this, it has been shrewdly insinuated that the oxygen produces these preposterous and out-of-the-way effects only, when it is brought into action in a *nascent state*.—Happy, thrice happy the genius which suggested so fortunate a salvo, in so embarrassing a dilemma !

Oxygen then, in its *nascent state*, is quite a different thing from oxygen *a day old* !

But, if oxygen be oxygen, and nothing else but oxygen, it still remains to be explained, why, and in what respect, pure oxygen, *just separated* from a body, *differs* from *itself*, being still *pure oxygen*, at the *minute's end* ?

Here, for the present, I shall drop the unpleasant employment of finding out faults and conjuring up difficulties. I must assert, however, in the strongest terms, that there is nothing invidious in any of the transient remarks I have made ; on the contrary, I have the highest veneration for every name I have mentioned ; and



esteem their invaluable labours as justly entitling them to the highest honour, and to everlasting fame! To equal celebrity and respect I think some few of the French chemists are justly entitled; at the head of which list is the name of *Lavoisier*; than which none is greater at this day, nor has a fairer claim to pass with honour through ages yet to come! But no name, however great, can sanction *error*: and it certainly is meritorious in any man to expose *it*, whenever he discovers *it*; that its baneful influence may be destroyed, that truth be no longer obscured by its shade: and my chief motive for bringing forward these difficulties is, that by fair reasoning they may be explained away; or, that principles, which *cannot* be *supported* by *reason*, may *not* be thought worth *defending* by *argument*; but, that sounder principles be sought for to supply their place.

# PRACTICAL INFORMATION,

&c. &c.

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## *The History and Description of the Disease.*

EARLY in 1801, several cases of fore-throat occurred, which were neither dangerous nor peculiarly obstinate. In July many children were seized with sickness, succeeded by fever, which in a few days was attended with an universal eruption, which by their mothers and nurses was supposed to be the measles; some degree of soreness of throat at the same time was generally complained of. All these symptoms in a few days, most commonly, disappeared, with-

out medical assistance being thought necessary.

It was soon, however, found, that the disorder was not the measles, as the fever and soreness of throat, in some cases, were particularly distressing, while the eruption was scarcely visible; and several children, who had lately had the measles, were covered with a florid efflorescence, which after a few days disappeared, when the cuticle gradually peeled off in scales like bran; and it now was found to be infectious, as it commonly seized upon all the children in the family, and spread from house to house; few entirely escaping, where there were children, in the neighbourhood. Some few amongst the lower classes died, probably owing to mismanagement: as hitherto there had been no signs of much malignity. I say probably from mismanagement, as it is so common a practice with the poor in cases of sickness to beg wine and ardent spirits of their affluent neighbours, which they administer to the sufferers in all cases indiscriminately. I cannot mention this circumstance without



without at the same time expressing a wish, that those whose benevolence renders them subject to such applications, would never give either wine or strong liquors of any kind, till they have, what they at least think to be, sufficient reasons for believing that such things will not be injurious; but, if a medical man is employed, never without his approbation.

In August the disorder became really dangerous and truly alarming; the fever, efflorescence, and fore-throat spread rapidly, and was no longer confined to children: as now young adults of both sexes were subject to its ravages; nay, in some cases, but they were not numerous, those who had arrived to the ages of forty, and even fifty years, were attacked, and that, too, with extreme violence.

The throat, now, frequently became highly inflamed, swelled almost to suffocation, and frequently ulcerated and highly foetid; the parotids on each side also being greatly enlarged and very painful.

But

But the most distressing and dangerous symptom was an affection of the brain, which, particularly in adults, was productive of scenes of horror indescribably distressing ! In these cases the fever was high ; the skin flushed, but not always efflorescent ; the pulse rapid ; the throat commonly highly inflamed, often ulcerated and swelled till breathing was an herculean labour : but, the raging madness of delirium ! oh, it was inconceivably horrid !!! Frequently it required the strength of three or four men to keep the patient in bed : if for a moment left unguarded, he threw himself upon the floor, and rolled around in agony ; and, if a female, was regardless of modesty and insensible to shame. Sometimes the most mischievous rage possessed them, and their nearest and dearest friends were not exempted from abuse ; while the loud ravings of frantic wildness, or the wailings of distress, assailed the ears, and struck terror to the hearts of those who passed before the houses which contained the miserable, the wretched sufferers !

As may be supposed, unless the violence of the disorder was speedily subdued, a horrid death was shortly the inevitable consequence; when a high degree of putrefaction rapidly succeeded, and, not unfrequently, considerable quantities of blood escaped from the distended vessels, and poured out by various passages.

The pulse was always rapid; the breathing quick, laborious, and interrupted by frequent sighings; the sensation of distressing oppression about the præcordia was anxiously complained of; and the dread of suffocation from the great distention of the vessels about the throat, increased the misery to the highest degree of agony.

Towards the close of this scene of wretched suffering, children commonly became convulsed, and adults not unfrequently.

A variety of other symptoms occasionally attended to add to the distress; but, as they were  
neither



neither constant, common, nor essential to the disorder, I shall not stop to consider them.

About this time several, particularly children, speedily fell victims to its malignancy; some being taken off in eight and forty, six and thirty, or even, in some few cases, within four and twenty hours from the time when the attack first became sensibly evident! In one village, as I was afterwards informed, between forty and fifty died of the disorder within the space of one month: a fatal proof of its prevalence and malignancy!!!

The duration of the disorder, whether it terminated in health or death, was very various. Some were taken off rapidly; some struggled to the sixth or eighth day; and some even longer, before their powers were finally extinguished. In slighter cases patients commonly recovered in six or eight days; but in more serious attacks, the time of the disappearance of the infection was extremely uncertain; and, when the brain had been considerably affected, stupor, fatuity, and

and in some cases a certain degree of general paralysis were the unhappy consequences. Sometimes foetid ulcers appeared, and in several the extremities were much swelled, and remained so after the fever was subdued.

In August and September the contagion was most virulent and general, and consequently most fatal; after that its influence gradually diminished. But, even in the month of March 1802, several cases occurred; and at this moment, June 1802, it is not totally extinguished, although it now frequently takes quite a different form.

Having thus given a general history of the rise and progress of this alarming, distressing, and fatal contagion, and of its usual modes of attack, characteristic symptoms, and various terminations, as it appeared in the neighbourhood where I reside, I shall avoid being tedious by entering into a detail of minuter and less important appearances, and shall immediately proceed to the mode of treatment.

*Method of treating the Disorder.*

The first appearance of the disease was not marked with any symptoms of violence or danger; few died. And as many readily recovered when the constitution was unafflicted by medicines, and many under different modes of treatment, it is not clear that the medicines which were applied were of any real utility.

In August, when the infection became peculiarly virulent, and rapidly spread its baneful influence around; when death and horror joined company with the disease, and wild alarm was painted upon every countenance, it then became truly a serious consideration, and the most strict and unremitted attention became absolutely necessary, that the most effectual mode of treatment might as soon as possible be ascertained.

I found *by repeated inquiries* respecting the progress of the disorder, and the fate of those who were seized with it in the adjacent



cent villages, that bleeding, blistering, emetics, purgatives, salines, sudorifics, acids, &c. with gargarisms of various kinds, had been resorted to. Some recovered; some died; but none were sensible of having been really benefited by the means which had been employed; and it was soon strikingly evident to the inhabitants around, that the proportion of recoveries by those modes of treatment, were *not greater* than when the disease was entirely left to the constitution; and that the violence of the symptoms and duration of the disease were not *sensibly affected* by any thing which had been done to relieve them; therefore great numbers chose to trust to nature only.

I now was applied to by several labouring under the disease in an alarming degree, and in a few days had twelve patients, chiefly adults, besides many more slightly affected. Nine of them had fever and efflorescence, with violent inflammation and swelling of the throat; the other three, in addition to the above complaints,

were attended with great affection of the brain, and constant and high delirium.

I, too, had recourse to the established modes of treatment, the effects of which I attended to with unremitted application; but I could not perceive that any medicine which I applied possessed the least power over the disorder, either in mitigating its violence, or hastening its disappearance.

The three patients who were so highly delirious became worse! I changed my medicines; I tried every powerful medicine that was likely to succeed; I ransacked my library for precedents, and consulted all the authors of celebrity who had treated on such disorders; but still not any one article, or any combination of medicines, had any evident or decided effect in either moderating or removing the disease!

Now, some of the patients of the nine who had not any considerable delirium, slowly recovered. One of the three in whom the brain had  
been

been so greatly affected, in defiance of all the pains I had taken to relieve him, died ! and a second, at the same time, shewed signs of recovery. As the third had been attacked later than the other two, and as the modes of treatment in the two former were not exactly the same, I immediately put him under the same course of medicine as had been administered to the recovering patient ; but, alas ! with no good effect, as he also fell a victim to the disease on the second day after !

The others slowly and difficultly recovered ; but after a most impartial review of every circumstance, and a fair comparison of these cases with the general progress of the disease when left to nature, or under different modes of treatment, I was under the necessity of drawing this mortifying conclusion, that all my attention, and all the medicines, however various, or in whatever manner combined, which I had hitherto given, had *not* been *productive* of *any* good effect *whatever* !



At this very moment, when all my efforts were baffled ! my confidence destroyed ! and hope itself almost extinguished by the distressing load of anxiety which oppressed my mind, the disorder became still more formidable, more frequent, and more commonly alarming ! Every day new names were added to the list of sufferers ; and it was not without the most mortifying and painful sensations that I saw them fly with confidence to me for relief, at that very time when experience had just taught me by the clearest evidence, that *small indeed* was the *assistance* in my power to give them ; unless by a *bold deviation* from the established modes of practice, an *effectual remedy* could be found.

For some time past I have not only made so free as to think for myself, but have also ventured to think very differently from others upon many subjects. The nature of contagious diseases was one of those subjects ; and as my ideas of the cause were widely different from the common opinion, so also is the practice which those novel ideas suggested.

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The remedy which my ideas insinuated as most adapted to the removal or destruction of contagion like this, however, was merely *theoretical*; and as such, I could not think myself authorized to try experiments with it, while established modes of practice were successful. But by the deaths of these two patients, and the decided inefficacy in every other case of severe affection, I was now perfectly convinced that those *established modes* of treating the present disorder were neither *successful*, nor in the least to be *depended* upon; and therefore not only thought myself authorized to change, but also fortunate in having any promising resource, in so distressing a conjuncture, to fly to.

In April 1800, I had had several patients in a *typhus fever*, which bore some slight resemblance to this, and which evidently seemed to be communicable. It was not, however, either peculiarly dangerous, much extended, or long before it disappeared. The usual modes of treatment were, at that time, very unsatisfactory; and towards the decline of the fever, I determined to try the very remedy which I since have had  
recourse

recourse to. I gave it in three or four cases; but the patients recovered so immediately after, that I could not think myself justifiable in concluding that the medicine had been any way concerned in their recovery; and as no more cases of that kind appeared either then or since, of course the real powers of the remedy were still unascertained.

Immediately, therefore, after the death of the second patient already mentioned, I determined that the next case which proceeded, unchecked by the usual remedies, to become really dangerous, or rather to become alarming, should be subjected to the *new mode of treatment entirely*, which I was resolved to administer boldly; but, as the medicine in which my only hopes were founded was so diametrically opposite in its qualities to those which are universally esteemed proper in raging fevers, I, at the same time, determined to give it with prudence and caution.

It was evening: my resolution had not been fixed one hour, when two messengers arrived  
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from



from two of my patients. The first was a gentleman upwards of fifty; the other a married woman, the mother of a family, aged about thirty. Each of them had been seized with a violent attack of the disorder about five days before: the first had been my patient three days, the latter two.

The symptoms were very similar, each having slight efflorescence; the throat much swelled and inflamed; and in both the fever had been high from the first, with great anxiety of mind and restlessness.

As their disorders had been similar in their progress, so were their situations now; as the information I received was, that in each the fever was greatly increased, and both were restless and delirious. None of the usual medicines which had been given in either case had been productive of any sensible effect upon the disease, which proceeded equally, and with unremitting or rather increasing violence in both!

My resolution was already taken; and the moment was soon to arrive which would determine the fate of my reasonings, my remedy, and my patients: nay, not only of these, but probably of many beside them; as numbers kept daily applying, either for the first time, or for more efficacious remedies than those they had already tried in vain!

Without hesitation, therefore, I dissolved *two drachms of volatile alkali, or carbonate of ammonia, in five ounces of water*: half of which solution was distributed to each patient, with orders to take *half a table-spoonful, or two tea-spoonfuls, every two, three, or four hours, according to the urgency of the symptoms*. If the difficulty of swallowing abated, and the patient wished it, a little cold water might be added to each dose. Cold water, or toast and water, to be drank at pleasure. I particularly requested that I might be informed of the state of both these patients on the following morning; and it was not without considerable agitation, produced by contending hopes and fears, that I saw the messengers arrive.

Conceive

Conceive then what was my surprise ! how great my pleasure ! how extreme my satisfaction ! when I was informed that each of my patients had found astonishing relief, even from the very first dose of the medicine ! that both had had several hours of refreshing sleep, the first they had enjoyed since the commencement of the disorder ; and, that both were cool and perfectly collected, having had nothing like delirium after the first dose had been taken half an hour.

Now had I reason to hope that I was possessed of a remedy, which seemed to be endowed with a *specific power* over the disease ; as in these two cases it had manifested an immediate action, by extinguishing the fever and soothing the mental agitation into perfect composure. In short, it gave me pleasure to find that these two patients were snatched as it were from a danger, which experience had taught me the usual modes of treatment were by no means of any efficacy in removing ; and the most exhilarating hopes that my other patients would soon experience the salutary effects of the remedy, and confirm its

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power



power by becoming happy evidences of its success !

Nor were my hopes disappointed ! — From this time the volatile alkali was my constant remedy in every state, every form, and every stage of the disease.

Some were glowing with universal efflorescence ; in some the extremities were swelled ; in others foetid ulcers appeared, particularly about the parts of generation ; in most the throat was swelled and inflamed, often ulcerated, and respiration almost prevented : but, in the most alarming cases, a scorching fever, and raging delirium, rendered the patient's situation horribly alarming ; yet, in all these variations of the disease, the volatile alkali was my specific remedy, which I administered to between two and three hundred patients successively and successfully.

In fact, although a great number of those patients were afflicted with the very worst symptoms of the disorder ; and although many of them

them did not apply till the disease had gained its utmost virulence, yet, under both those disadvantages, out of near three hundred who took the volatile alkali, but two died. Both were very young children:—in both the parts about the throat and nose were extremely swelled and ulcerated:—in neither of them was the solution given in such quantity as was *likely* to succeed:—in both the virulency of the infection was extreme, and far advanced before relief was applied for; one of them having been six days before application, and the throat was become so foetid, and the child so generally and constantly convulsed, that I declared its recovery impossible; and only gave the medicine to satisfy the parents, without any expectation of its being of benefit to the child.

In recent cases, the first or second dose very frequently entirely destroyed every appearance of disorder: in most cases its beneficial influence was more or less sensibly perceived from the very first; and the satisfaction which my patients in general expressed, when they came to inform  
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me of its operation, was not less than my confidence in its powers, and my heartfelt pleasure at its success.

I mean it not to be understood, neither indeed can it be supposed by any intelligent person, that the volatile alkali was the *only medicine* which was administered in all these cases. In the greatest number of them, however, it was the *sole remedy* which was given, and all that was found necessary to the speedy and perfect removal of the disease, and the re-establishment of health; and in every other of these cases, its powerful and beneficial effects in removing the fever, &c. were not less evident. But peculiar circumstances and constitutions were liable to various distressing symptoms which were *excited* by the disease, but by no means *essential* to it, or its constant attendants; and these accidental symptoms, in their turn, kept up the disorder, and required particular attention, as well as the original disease; that, as common disturbers, they might be expelled together.



As my only intention at present is to point out to others the remedy which has proved so singularly and extensively successful in the scarlatina maligna vel anginosa in my private practice ; I shall not enter into a specification either of the varieties of those accidental symptoms, or the kinds of medicines which were necessarily combined with the solution to remove them. Whatever be the disorder which a patient is attacked with ; howsoever well known or common, it is, at all times, to his interest, his safety, and satisfaction, to apply to the *most intelligent and experienced practitioner* within his reach ; and to such this specification would be unnecessary. On this head I will only further observe, that as I never found any advantage was derived from either bleeding, blistering, emetics, cathartics, diuretics, sudorifics, opium, bark, acids, or any kind of medicine whatever, besides the volatile alkali, when the disease was *simple* ; of course, I never had recourse to any other remedy, until I was convinced that the *contagion* was *destroyed*. Then, if troublesome symptoms attended, I immediately *conjoined* such medicines as I thought necessary

necessary to remove them; when the symptoms, and the disorder, which they kept up by increasing the general distress, disappeared together; or, if stupor, fatuity, anasarca, general debility, or even delirium, without any remaining fever, as frequently happened, were consequences; they readily yielded to appropriate remedies, without any difficulty that I ever experienced.

Having thus faithfully laid open my experience; the new mode of treatment, or rather the new remedy I have adopted, and the great and constant success which has attended it in nearly three hundred cases in every stage, every form, and every degree of virulency of this alarming, this distressing, and, alas! too frequently fatal disorder; I shall only add my most sincere wishes that the *experience of others* may confirm the *hope*, which is founded on *my own*, that the remedy I now recommend is possessed of specific-powers in the cure of this, if not of other malignant disorders, like it, arising from contagion; at the same time declaring, that such have been the effects of this simple medicine, that were I

to be seized with the *plague itself* this moment, the *volatile alkali* would be the only remedy I would have recourse to, and I should fly to it with *confidence*!

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*A few Remarks, by Anticipation, on the Conclusion which will probably be drawn of the Nature of the Disorder from the Quality of the Remedy.*

As chemistry is the fashionable study of the day; if I be favoured with half a score readers, seven of them must be chemists; for at this time French principles are as necessary to the philosopher as French fashions are to a fine lady: neither of them can safely appear in public without them; consequently, as being out of the fashion is generally considered as like being out of the world, every man who wishes to be thought *knowing*, must be a chemist; and a chemist too of the antiphlogistic order.

One common remark and general conclusion will naturally occur to the gentlemen of this  
 F order;



order ; which, as nearly as I can predict, will be in the following words :—“ Umph !—If this be a fact, that the carbonate of ammonia counteracts, or destroys the contagion of scarlatina maligna ; then, it must incontestably follow, that this *contagion* is an *acid*, and that the disorder is in reality a sur-oxygenation of the system, which the carbonate of ammonia destroys, or removes, by combining with that acid to neutralization, and thus depriving it of its influence on the system ; by which means the system is relieved from the disorder occasioned by the excess of oxygen, and health is restored.”

If this be a fact, that the carbonate of ammonia is a successful remedy in the scarlatina maligna, can any man be dissatisfied with so natural a conclusion : a conclusion so pat and so truly chemical, that it seems to carry the fullest conviction along with it ?—And yet there are objections, nay powerful objections to such conclusion, and to any explanation which the modern principles of chemistry, consistently with themselves, can give.

It does not absolutely follow that this contagion is an acid, because ammonia destroys the effects it produces, and becomes neutralized by its passage through the body; neither is such a mode of arguing either consistent with reason or facts. For example, alcohol will combine with oxygen gas so as to form water. Many good old ladies are subject to *colicky pains* which are always *relieved* by a few glasses of *Nantz*, whose efficacy resides in its *alcohol*: and this *Nantz* is converted into *water*. But are we then to conclude, that whenever one of these good old creatures has a touch of the colic, it proceeds from her having *fifteen* or *twenty* gallons of *oxygen gas* in her belly? Surely not. Though gas of some kind or other is no uncommon attendant in such cases, as is frequently evident to more senses than one; yet, by whatever passage it makes its escape, it has never struck me as being of the *empyreal kind*.

If the symptoms in scarlatina maligna are the effects of hyper-oxygenation, it is surprising that they have not been constant attendants in

many hospitals, and in many circles of private practice of late; seeing it has been so very much the *fashion* to give the acids to an excess not known in former times; but, though hundreds and thousands of patients have been super-oxygenated to the highest degree, yet, I never heard they were attacked with the symptoms of scarlatina maligna in consequence. Neither does super-oxygenation render a person more *liable* to the disease, as I have had more than one indubitable proof of, during the time of the ravages of the disorder now under consideration. Nay, so far from that, in three cases particularly, where the patients, otherwise in perfect health, were taking acids liberally for a cutaneous disorder, *they escaped* the contagion, though their companions, who accompanied each of them in an occasional attendance on the sick, were *every one* of them *seized with the disease* on the third or fourth day after.

It is very evident then, that super-oxygenation neither constitutes the disorder, nor predisposes the body to become subject to its influence.

How



How then can the contagion be an acid? and if it be, what is the state of that acid?

Recourse cannot be had here to the ridiculous argument of nascent oxygen; because, if nascent oxygen means any thing, it must be oxygen in the act of separation from one body, transferring itself to another; which of course must be immediately in *contact*. But breathing the air of a room where a patient in this disorder is confined, without coming within three feet of the patient, has, in many cases, incontestably, communicated the disease; which, on the third or fourth day after such exposure, hath so constantly appeared, as to render the time and mode of infection no longer problematical. Here the oxygen, if oxygen must be the cause of the disorder, had not only been *separated* from the infected body *some time*, but had *floated* in the atmosphere to the *distance* of *three feet* at least: it therefore could not be *nascent*, strictly speaking, but was as *free* and *independent* as though it had been separated *three years*, and had been *wasted* round the *globe*.

Perhaps

Perhaps it will be said that this acid of contagion is not simply oxygen, but a *combination* of oxygen with a *peculiar base*, forming a peculiar acid, possessing the peculiar properties, to which we give the name of *contagion*; and that this acid of contagion has another peculiarity, which is, that, when received into the blood and acting as a contagion, it causes a *great quantity* of a similar acid of contagion to be *generated*; as is peculiarly evident in small-pox, from inoculation; when the *hundredth part* of a grain of the matter of infection will frequently produce as much infectious matter, *absolutely similar in all respects*, as can be contained in the *large pustules covering the whole surface of the body*!

Admitting then that the matter of contagion is an acid, formed of oxygen and something else; still, that oxygen is not saturated with its base; for oxygen saturated is no longer acid: it still then acts as oxygen, and is in reality oxygen, only partly or imperfectly combined with some peculiar base.

If then a small portion of this acid of contagion be introduced into the circulation of a person in health, it causes an inconceivably greater proportion of a similar acid of contagion to be generated; that is, it causes the *oxygen in the healthy blood* to combine partially with some *other principle of the blood*; and by forming this *combination* to separate from the other principles of that blood they before were united with: for, as the person is in health, and the blood in a healthy state, when the contagion is received; and as nothing but what is natural and conducive to health is taken in afterwards, till the contagious acid is formed, it is evident, that the *acid of contagion thus generated*, is actually *formed from certain principles separated from the pure healthy blood itself*, of which they were a constituent part.

The question now is, how does the oxygen of contagion *separate* the oxygen of the blood from all its other combinations, except that portion of something or other which renders it contagious also? By what law of chemical agency are we



to explain in what manner *one grain* of oxygen, combined with a certain base, acts upon the other principles of the mass of blood in general, so as to make them *deposit a thousand grains* of *similar contagion*, formed of oxygen with a certain base? In short, why healthy blood, by the accession of a *small portion* of oxygen in a peculiar state, should be immediately disposed to *deposit an immensely larger proportion* of oxygen in a similar state?

These are questions, which the play of simple affinities between base and base, appears to me by no means calculated to answer; and consequently the theory which is founded upon such principles is not capable of giving that satisfaction which an inquisitive mind must desire, and ought diligently to seek after.

If oxygen, a name which originated in error, be a simple principle, it ought to be consistent with itself. If then the matter of contagion be an acid, or a modification of oxygen, its properties ought to bear a resemblance to the gene-

ral properties of all other acids or compounds where oxygen is not in a state of saturation with its bases: but that is not the case; for the acid of contagion causes a generation of a similar acid in an astonishingly great proportion to the original exciting acid. This then is a striking peculiarity which no other acid is possessed of. Indeed, were other acids endowed with the same generative powers when received into the living body, *some practitioners* of the present day might make *fortunes*, by merely preserving the *droppings* from their patients.

If the matter of contagion be an acid, which produces the various symptoms of scarlatina maligna when it abounds in the system, other acids, which are mere compounds of the same oxygenous principle in excess, when they are thrown into the system in great quantities, ought to produce violent actions similar to them: but they do not; and therefore, to say that contagion is an acid, and that its effects are produced by sur-oxygenation of the system, is saying nothing.

In short, if oxygen be a simple principle, which, in its simplest, most disengaged, and most condensed state, is *innocuous* when properly diluted, how does that inoffensive principle become *virulent*, when its powers are *partly exerted in affinity* with a base of any kind? If the *full exertion* of its powers be *harmless*, how can a *partial exertion* of those identical powers be *destructive*?

The same mode of reasoning applies to the other principle, as a base of contagion. It is evidently derived from the blood; but every principle in the healthy blood, in its simplest and purest state, is innocent, when taken into the body; how then can one simple *salutary* principle, by combining with oxygen, which is *equally* so, acquire *deleterious* properties, or form a compound which is possessed of *destructive powers*, when that combination is merely the effect of the simple principles *attracting each other*? By that attraction they cannot gain any power, or any new property; for when two bodies attract each other, *a part*, at least, of their powers  
are



are mutually exerted upon each other; and, consequently, the only effect that combination can produce upon the powers of bodies or molecules, is, that as those powers are in a great measure expended by acting upon each other, each body will have less power to act with upon other bodies.

Perhaps it will be alleged that the *bland* or *malignant* qualities of oxygen, or of the composition into which it enters, depends upon the quantity of *caloric* combined or connected with it:—indeed!

Oxygen, then, with a full dose of caloric, constitutes oxygen gas, according to the modern doctrines. But, as that is the purest and most salutiferous of all gases, it *cannot* be supposed that the oxygen in the state of contagion has any *surplus* of caloric; that would be deviating on the side of salubrity, and its effects ought to be highly invigorating, and similar to those produced by inhaling oxygen gas: therefore, as the effects of contagion are widely different, of course,

course, if caloric has any thing to do in the business, the *oxygen*, in *contagion*, must have *less* than its common proportion of it.

But, when oxygen leaves the infected body to pass into the atmosphere, why does it not take a sufficient quantity of caloric along with it, as there can be no *scarcity* of it when it separates from the *inflamed* surface of a body *scorching* with fever?

Probably by way of saying something, or, in short, any thing rather than give up a favourite idea, howsoever absurd, it may be urged that the oxygen, forming the acid of contagion, is *deprived of its caloric* by its intimate combination with the base of that contagion; or that the acid of contagion is formed by the combination of oxygen with the peculiar base, *both* of which are *deprived* of their *caloric*.—Then the question is, how is it possible for *one single grain* of the acid of contagion to deprive *a thousand grains* of oxygen, or of the *base of contagion*, or of *both*, of their *full proportion of caloric* which they *possess* in the  
*healthy*

*healthy blood*, and thus to *precipitate* them from the common mass, in the *form* and with all the *virulent properties* of *contagion*?

Here, however, I shall drop the pursuit; having brought forwards a sufficient number of objections to the modern modes of explaining the nature of diseases, and the operations of medicines, to apologize for my *rejecting* them, and for my *boldly daring to think for myself*; for, while such objections remain in full force, he must be fond of a theory indeed who can be pleased or satisfied with it; and they *must remain in full force* till they are *explained away* in a *rational and satisfactory manner*.

The greater number of those who pretend to philosophical knowledge are men who are not capable of thinking deeply (some from incapacity, and others from habitual indolence, and the degrading custom of swallowing the *literary evacuations* of men of really great or pretended abilities, as *well-digested opinions*, without considering for a moment whether they are either *sound* or *wholesome*); and for that reason they

2 are



are perfectly satisfied with any arguments which appear plausible, howsoever superficial. With philosophers of this stamp, when opinion is once formed, every attempt to alter their mode of thinking, or root out that opinion, howsoever erroneous, is absolutely in vain—they have made up their minds : they are satisfied that they are right : the greatest philosophers of the age have advanced and still maintain that opinion ; then, who can produce greater authority for a contrary opinion ? If you attempt to reason closely upon the subject, they wisely turn a deaf ear :—for, if they listen they cannot understand ; or *objections* are *not attended to*, because they feel they have it *not in their power to answer them*.

I don't like principles that are too easy, too comprehensive, too good-natured, and too ready to do or undo, just as occasion suits. Every man who reasons, ought to have principles upon which his reasoning is founded ; but those principles ought to be simple in themselves, simple in their properties, constant in their effects, and at all times, consistent in their operations. But  
when

when principles are assumed which are hot or cold, fat or lean, wet or dry, consistent or inconsistent, whichever is the most convenient at the time; such ideal principles, or rather such *terms without ideas*, are not the permanent, unchangeable principles of a philosopher, but the *playthings of a child*; a *whistle* at one end, a *rattle* at the other!

Philosophy is the fashionable folly of the day, but what philosophy? Is it the love of wisdom, of knowledge, of facts converted into science by reason? No. The philosophy of the present day consists in one continual waste of useful articles, in trying ten thousand trifling experiments, and repeating them ten thousand times over, and ten thousand times laying the jarring results before each other, and the public.

Let it not be thought, however, that I am an enemy to experiments, or ignorant of the great advantages which have been derived from them; on the contrary, no one is a greater advocate for them, when conducted with judgment, than myself; but, still, *experiments* can but furnish *materials*,

*rials*, which *reason* must convert into *order* and *science*. *Experiments* are *steps* by which the philosopher *ascends* for the purpose of gaining a *more extensive and diversified view* of what surrounds him; but those steps, however high they be piled, are *not wisdom*: and the man who spends himself in heaping step upon step, is the mere *slave of science*, who erects a *tower* from whose *summit* the *true philosopher* contemplates at his leisure the wonders of nature, thus *widely opened* before him.

I think I hear some one exclaim, What! are the principles of the French philosophy so universally admired, and so generally adopted, are they not simple, consistent, and intelligible? and is the beautiful theory so full of explanation, which is founded upon those principles, a detail of experiments without reasoning? Softly, and you shall hear, my good antiphlogistian! I have, so far, never advanced an opinion different from the common every-body's-way-of-thinking, without giving my reasons for it; and as reasons to support such an opinion are ready at every turn,

if



if you will give me leave, I will lay before you the first which come to hand.

As oxygen is a leading feature in almost every experiment which occurs, nothing more readily catches the eye than it. Let me then for a moment attend to what every body says, and every body believes, and every body is ready to maintain (except when they meet with arguments which may prove troublesome) concerning the principle called oxygen.

*Oxygen* is a simple principle, according to the French school, which possesses very singular properties. Sometimes it forms by combination with *bland* and *mild* principles, the most *acid* compounds : as, for example, when by combination with *azote*, it forms *nitrous acid*.

Sometimes it combines with other mild substances, *without any acrimony* ; as when with *hydrogen* it forms *water* : and sometimes with the most *acid*, *penetrating*, and *corrosive* of all things, it forms a *mild compound* of the most grateful and

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salutary

salutary qualities ; as when with caloric it forms *pure air*, or *oxygen gas* !

All *gases*, somehow or other, are *chiefly* composed of *fire* ; and they take their names from the *small*, the *very small* proportion of the *principle* which in this somehow-or-other-way is combined with it ; thus two or three grains of oxygen with a pint of caloric, form a pint of oxygen gas ; and a grain or two of azote, with the same measure of caloric, constitute a pint of azotic gas.

Oxygen and azote thus by caloric converted into gases, have *no power of combination*, as is evident by mixing them together ; when they stand all weathers, and every possible degree of compression, without combining.

Nitrous acid is formed of oxygen and azote. If to this acid there be put a few filings of iron or copper, simple bodies, as the French teach us, the metal will attract part of the oxygen, when the azote, by means of caloric, expands into the state of gas, taking a portion of oxygen along with it.

If this azotic gas, *charged with a portion of oxygen*, be mixed with oxygen gas, it will rapidly seize its oxygen; and the azote, by combination with that oxygen, and with that already attached to it, will form nitrous acid.

Or if this azotic gas, charged with a little oxygen, be added to a mixture of azotic and oxygenous gases, it will take the oxygen gas from the azotic, and with it will form nitrous acid; that is, they say, the azote not being saturated with oxygen in the state of nitrous gas, will not only seize the oxygen of oxygen gas to saturation, but it will take *more* than it can *saturate*, and thus form a compound where oxygen evidently abounds; and this combination is not in any respect interrupted by each of those principles being combined with caloric, and in the state of gas.

Yet, a little while ago we were told, that azote and oxygen in the *state of gases*, would *not combine*; and now we find, that in the *state of gases* they *will rapidly combine*.



What reason is assignable for this palpable contradiction ? The difference is, that *azote* in the state of *nitrous gas*, is *already combined* with a *small portion* of *oxygen*, and with *caloric* sufficient to give it the state of gas ; but in simple azotic gas, the *azote* is *only combined* with *caloric* ; if it can be called combination, where one thousandth part of the caloric cannot be in contact with the azote.

Then *azote with caloric*, has *no sensible action* on the *oxygen of oxygen gas* ; but if that azote with its caloric, be by any means *combined* with a *portion of oxygen*, it then becomes so *keen*, so *voracious*, so *insatiable in its appetite to oxygen*, that it will *devour more* than it can *digest* ; will *seize upon more* than it can *saturate*, in *despite of caloric*, and thus will constitute nitrous acid ! Azotic gas, *already partly combined with oxygen*, then will attract oxygen in the state of gas *rapidly* ; while azotic gas itself has *no sensible attraction whatever* to oxygen in the state of gas !

Surely.

Surely it is a little extraordinary, a little irregular, a little inconsistent in azote, that, when combined with caloric in the state of gas, it will not enter into combination with oxygen gas ; but if by any means it be *crammed with a little oxygen*, when combined with caloric, its appetite then becomes so insatiably voracious, that it will *gorge itself* with oxygen to *super-saturation*. Yet, *such are the doctrines, such the principles, such the beautiful theories, so full of explanation, so universally admired, and so generally adopted ! and such the philosophy of the day ! !*

It certainly is a matter of little consequence to me, what are the prevailing opinions of the multitude ; yet, I feel myself so sincerely attached to truth, and so devoted to her service, that I cannot avoid occasionally pointing out what *appear to me*, evidently to be *errors* ; that they may be carefully *investigated*, and, if really errors—expunged : this, and this only, has been the motive which has induced me to write what I now offer on the subject ; and I freely confess that I have undertaken it rather as a duty, than as hav-

ing any expectations that what I have written will be in *any degree attended to*, for reasons which I have already more than once insinuated.

Be that as it may; being from long and full experience convinced that the generally adopted modes of practice are neither founded on sound principles, nor of such decided efficacy in the greater number of disorders as to inspire the experienced practitioner with confidence; and being fully persuaded that great improvements might and *ought* to be made, I for some time past have devoted all my time and all my attention to the subject, and my labours have not been wholly in vain.

Although it would certainly be greatly to my pecuniary advantage to keep as secrets the improvements which my experience persuades me I have made in the treatment of some of the most formidable disorders, yet no private views shall interfere with my first intention, which was, to direct all my endeavours to one great end—*the public good!*

This,



This, then, is the first communication, and I with confidence hope that it will be found of importance.

In a short time it is my intention to lay before the public new methods of treating *inflammation of the bowels and strangulated hernia*.

The unfortunate event which has so lately deprived the world of one of its noblest and greatest ornaments, makes me regret that the mode of treatment which has so constantly succeeded in some of the most alarming cases of the above disorders which have occurred in my private practice, was not then communicated, as the practice is not only new, but experience has taught me, that beyond every other that I have heard of, it is also efficacious.

As this is the first of an intended series of communications, I have thought it not improper to give this slight view of the reasons which have compelled me to abandon the commonly-received theories, and now shall dismiss the subject;

ject; as, without allusion to the theories of others, or to opinions of my own, I propose that my future communications shall be entirely *practical*, that being the readiest way of accomplishing my wish—to be useful.

THE END.

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